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SCIENTIFIC PROGRESS AND POLITICAL SCIENCE

Bernard Brodie

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The following paper, "Scientific Progress and Political Science," was prepared for the 1956 meetings, in New York City, of the American Association for the Advancement of Science. It is to be presented on the evening of December 26, 1956. It will also be published in the Spring issue of Scientific Monthly, and republished under the Special Publications Series of the National Academy.

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SCIENTIFIC PROGRESS AND POLITICAL SCIENCE

by Bernard Brodie

The topic assigned me, "Scientific Progress and Political Science," is broad enough to permit a wide variety of attacks; but I have chosen somewhat arbitrarily to deal with what is one of the most important problems facing political scientists today--namely, their deficiencies in coping with policy decisions affecting national security in an atomic age. I want to talk especially about some of the more important reasons why, in my opinion, we political scientists suffer these deficiencies.

In choosing this course, I am stimulated partly by the fact that the organization sponsoring these meetings is populated far more by physical and biological scientists than by social scientists. I presently work in an organization in which a comparable asymmetry exists, and I know that many of my RAND colleagues are often mystified by the ways of the social scientists. I therefore trust it will not be amiss if I try to explain to those of you in other fields the habits of that very diverse assortment of beings called "political scientists."

Let me first assure you that scientific progress in the large is something my political science colleagues are in favor of, as you other scientists are: though they, like you, sometimes fear the results of it. It is a singularity of our culture and our times, otherwise so material and profane,

that scientific progress should be universally approved of without qualification, at least by intellectuals, despite the extremely menacing nuclear monsters that have been spawned along the way. I can think at the moment of no other set of values less subject to challenge or exception, even if we confine ourselves to this side of the iron curtain.

The political scientist is also all in favor of scientific progress in his own field. He can well afford to be. He has a lot of ground to cover, and thus far his discoveries have been such as to occasion very little fear or anxiety. Some of them do have a certain nuisance value, like the discovery that by appropriate and most ingeniously selected samplings of the population, and proper treatment thereof, the tastes as well as the opinions of the general public can be fairly accurately polled. Even so, there is no proof that such discoveries have lowered the levels either of our television programs or our election campaign speeches. After all, the late H. L. Mencken made the observation long ago that no one ever lost a fortune underestimating the tastes of the American public (or any other public, he could have added). And although that comment lacked the quantification we would now consider essential for respectability, it was remembered, I think, more for its sagacity than its cynicism. Indeed, in so far as we are not obsessed by a need to achieve winning majorities or pluralities, the pollsters may have discovered for us that there is more intelligence and good taste

at large in the world than those who wish to cater to such qualities might otherwise assume.

Anyway, the political scientist worries much more about the slowness of development in his own field than about the possibly dangerous impact of the relatively simple-minded findings he has made thus far. He is acutely aware of the failure of his field to keep pace with the needs which a swiftly developing technology fed by advances in the physical sciences have imposed on society. The Presidential Address last September of the retiring president of the American Political Science Association, Professor Harold D. Lasswell, was devoted to that subject, with special reference to military technology.

I suppose also that in a more private recess of his soul the political scientist, like the social scientist generally, worries also about the fact that his findings and discoveries do not really register in the way that they do in the physical sciences. In the physical and biological sciences a significant finding is established as a factor to be reckoned with permanently by all workers in the field to which it pertains, at least until it is definitely superseded by some relevant but different finding. And there is something majestically impersonal about the march of events in the physical sciences. One is almost always able to say with assurance that if so-and-so had not made this particular key discovery at such and such a time, someone else would certainly have done so within a reasonably short time thereafter. Such a statement does

not detract from the credit of the discoverer, whose merit apparently lies less in his special uniqueness of insight than in his position at the van of a movement pressing against the boundaries of knowledge. Perhaps one should except special geniuses on the level of a Newton or an Einstein, but surely we can say even in the case of the former that the laws of motion and gravitation would be just as much a commonplace as they are today if Sir Isaac had never lived.

The situation is very different in political science, where much excellent work has proved to be writ in water. I am speaking of the fact that political science must to a large degree concern itself with the contemporary world, which means with a highly specialized and partly fortuitous set of circumstances, bound to change importantly within a short time. That is so even if the political scientist is primarily interested in the deeper currents of affairs over the long term, as I think he usually is. The things that are researchable and the questions on which his judgment will be sought, if at all, usually concern the highly specific present. Naturally, work of exceptional quality tends to have some enduring value in any case, partly for the example it sets for younger scholars and also because general insights into man as a political animal are expanded by such work. But these accretions are as a rule indirect and marginal relative to the dimensions of the work and of the original value, however temporary, of that work. Also, the author is not

likely to be remembered for it, because obviously with a vast pressure of new literature always upon us, few of us are going to bother with items that are conspicuously dated.

That last point leads me to another major quality that I think distinguishes political science from the physical sciences, and that is that its product is and has to be expository. Political science, like most other social science, is made of prose. It has been charged, I think justly, that it is usually bad prose, and I will not attempt to argue that the badness of the prose is necessary. But certainly the few attempts I have seen to introduce the symbolic language of mathematics into political science discourse have seemed forced, perverse, and usually ludicrous.

I am of course excepting statistical studies, in which for the moment we find it convenient to regard human beings as the strictly identical units which, in the remainder of our waking time, we political scientists are at special pains to point out they are not. But most of the things we regard as worth communicating require words for their communication, often very many words. The ideas we develop often differ from the utterly banal or obvious only in the special nuances we are able to apply and which we think meaningful and sometimes even important. Within certain limits the pruning away of words usually improves style and sometimes results in the elimination of nonsense, but beyond those limits it can make important new truths look like truisms.

The fact that political science is purveyed and absorbed only through the writing and reading of large quantities of words accounts for a good many idiosyncracies in the field that physical scientists find bewildering. We do not have in our field a tidy system of inventory for our ideas, such as in other fields makes for an orderly building of new knowledge upon old, with proper (for both moral and incentive reasons) attribution of credit. I do not wish to suggest that in this respect the situation is utterly chaotic in our field: only that it is considerably less tidy than what mathematicians or physical scientists are accustomed to, and that we pay a considerable price in lost or wasted motion for this untidiness.

Another large and unfortunate result of the wordiness of political science is the fragmentation of the field into divisions that may have very little contact with each other. The business of keeping up with the literature in one's field can at best be done only imperfectly, and even then only if one delimits one's field quite narrowly. I am speaking especially of those who are or try to be productive scholars.

The fragmentation cuts right through whatever theoretical structure exists in the field. It is not correct to say there is no theory in political science; there is a measure of empirically-based theory in each of the several branches of political science. What is true is that there is no body of theory common to the whole, from which the several specializations make their departure. What we generally call "political

"theory" is usually only a history of distinctive personal philosophies about political systems.

I must mention also the special kind of split-personality disorder which almost all political scientists are heir to. The political scientist over the years has schooled himself to a fairly austere conception of the proper scope and methods of research in his field. But he cannot escape, and usually does not want to escape, that aspect of his art which requires him to be a critic of public policy where that policy impinges on his specialty. In that respect his position is very different from that of the physical scientist, who criticizes policy, when he does, only as a citizen, and not as a specialist in a field where policy decisions are intrinsic to his subject matter.

Now it is a simple fact of life that some of the most baffling and most critically important issues requiring policy decisions, especially on the national or international level, are not in any direct way researchable. Problems may be formulated and investigated which are in some way relevant to the basic policy decisions, but often they are only tangentially so. Frequently the most we can do is discover and analyze a few historical analogies to a present problem, and the more exceptional and novel the present situation the more dubious is the value of such analogies.

Policy decisions after all involve predictions about the future, and must sometimes take account implicitly of a

considerable range of contingencies. The political scientist often can, as a result of the research he and his colleagues have already done, point out that some contingencies are much more probable than others, or that certain popular proposals have a demonstrably poor chance of success. These can be very important contributions, and the pity is that such analysis is not more regularly used where it is available. When done systematically and carefully, it becomes a kind of research in itself. The now familiar term "operations research," broadly conceived, would encompass such an activity.

Even so, on the major issues there usually remains a broad area for the exercise of something other than research talent, and the political scientist is not free to abandon this area. In so far as the policy decision in question impinges on a field in which he has a special competence, he may justly hold that his intuitions are likely to be sounder than those not similarly endowed with information and experience. Yet fundamentally his opinion concerning policy has to be accepted or rejected on the basis of some one else's intuitions about his abilities, because he can offer nothing which he himself would regard as a satisfactory proof of the correctness of his position.

Of course it is true that "experts often disagree," and nowhere are they likely to disagree more than in political science. But in the process of disagreeing scientifically trained experts are likely to expose sharply to each other

and to the world not only the content and the boundaries of the knowledge they consider relevant, but also the character of the logic by which they test relevance, meet each other's objections, and attempt to erect a connective tissue between known facts and the question to be decided. The fact is, also, that the experts generally disagree a good deal less than is commonly supposed, and that they have much more than average immunity to some of the sillier notions on the subject in hand that may be current.

Up to this point I have been talking about some characteristics--call them disabilities or limitations if you will--which I think are inseparable from the nature of political science. And let us always remember that the nature of political science is determined by a subject matter which is fundamentally different from the subject matter of the physical sciences in several crucial respects, especially in the transitoriness of the patterns which it studies. It is possible to establish certain universal propositions or "laws" in politics, but they have a tendency to be banal and uninteresting in direct proportion to the degree to which they are universal. Political science is no doubt more disorganized than it needs to be, and in its diverse departments more sloppy in method than it should be. It has been improving over the years, and will no doubt improve further. But there will never be anything comparable to a Gödel's Proof in political science, and it is childish to expect or demand that there should be.

Political science is fated to remain wordy and therefore fragmented; and it will always tend to be oriented round current policy problems. Within those limitations it will produce a lot of useful and even valuable work. At any rate, I am less concerned with the intrinsic value of the work than I am with the likelihood that the policy makers will pay little or no attention to it.

But now I come to a weakness which is not at all necessary and which demands to be remedied. It is a weakness that is not chargeable exclusively to political science but is pervasive in American intellectual life. However, I concede that political science has a greater obligation than any other single discipline to do something about it. I am referring to the fact that there exists in America hardly any tradition of intellectual concern with that increasingly wide border area where military problems and political ones meet. There are military historians, quite a lot of them, especially historians of the American Civil War. But there are exceedingly few scholars who consider it their primary business to inquire about the effects that current and projected military developments must have upon our politics, and vice versa.

This particular poverty in the intellectual life of our country is bound to be reflected in the world of affairs. Granted that ideally the military approach to strategic problems needs to be extended and leavened by the relevant insights of the statesman, such insights are likely to be

undeveloped among those real-life civilians with whom the military actually have to deal. There has been a great deal of advocacy of closer communion between politicians and soldiers in matters relating to the pursuit of foreign policy, especially that part of our foreign policy which has or may develop military overtones. This closer communion, unquestionably desirable, has been much less often urged on the grounds that the civilians involved might also have a beneficial influence on our military policy--a fact which reflects an almost universal consensus, in my opinion erroneous, that military affairs are inaccessible to the layman in a way that foreign affairs are not.

In any case, the problem is not simply one of achieving closer communion between two groups of men of markedly different training and orientation, but rather of developing a real competence on each side to penetrate and comprehend the issues with which the other side is currently seized. And if for no other reason than that the ultimate control of policy is in the hands of civilian leaders in government, it would seem reasonable to suppose that naivit  among the military on foreign and domestic politics might be much less harmful than naivit  among their civilian colleagues and superiors on military questions. In actual fact, however, if we were to ask which group really is the more naive about the other's problems, it seems to me clear that the booby prize has to go to the civilians vis-a-vis the military rather than the other way round.

The National Security Council is a monument to an aspiration, and the aspiration is undeniably sound. But whether any real enrichment of strategic thinking has proceeded from it is another question. Naturally, where the deliberations and conclusions of an organization are kept so highly classified one cannot be sure, but it seems clear from evidence available to the public that the NSC works much better as a medium for the military to impress their views on the civilians than the other way round--always excepting the matter of imposing budget ceilings on military expenditures, where arbitrariness generally rules.

There cannot be a real enrichment of strategic thinking unless and until considerable numbers of scholars in germane fields begin to concern themselves with the relevant issues. At the moment I am concerned mainly with the contribution that political scientists might make. There are a number of reasons why their contribution could be crucial.

Among these reasons is the fact that political scientists, especially that group of them who specialize in international affairs, tend to be concerned with the context of military operations in a way that the military themselves are not. The military officer is forced by the heavy professional demands of his craft to be preoccupied with tactical as against strategic matters, and to the relatively small degree that he concerns himself with the latter his interpretation of strategy is likely to be a restricted one. Clausewitz, himself a

general, pointed out a century and a quarter ago that the object of a war, which is always political and therefore appropriately determined by the politicians, must govern the whole conduct of that war; but this idea, while often alluded to in one fashion or another, has never really been absorbed and digested by the military profession. To the military the means available, rather than the object, are what determine the character of a war, and they have usually resented the "interference" of their civilian chiefs with respect to their choice of means.

In the two world wars the conflict between these two points of view was not particularly obvious. But the Korean war uncovered a deep and pervasive confusion on the matter of ends and means. The politicians restrained the soldiers' use of means because they spontaneously recognized that the true objects of American intervention required such restraints. On the other hand, largely because of the novelty of the situation, the political leaders were so inept at formulating and explicating those objectives that they made basic and even elementary errors of direction--above all the error of arresting their military pressure at the first moment that the Communists showed an interest in negotiations. The fact that the negotiations then dragged on for two years and resulted in a less than palatable truce is something that, because of the bitter distaste it left on the tongues of the American people, has deeply influenced two presidential elections.

Our people do not understand and certainly do not like a kind of war where we make relatively heavy sacrifices and yet appear to be committed to come off with something less than a clear victory. Yet "limited" or "peripheral" wars are by all odds the kind most likely to occur within the next decade or two, and thus the kind most likely to engage us if we become involved in military actions at all. Non-involvement may mean surrender of important positions, and certainly the only other alternative, all-out thermonuclear war, is an infinitely more grim and forbidding prospect than any kind of local war. There are good grounds for the common assumption that the former can be avoided--provided certain elementary precautions not commonly discussed are taken, such as providing for the greater security of our Strategic Air Command. But the posture that deters the enemy from all-out attack does little if anything to deter him from peripheral challenges.

In this connection I should like to point out that one of the most critical changes wrought by the atomic bomb is almost universally overlooked, and it is to our great peril that we continue to overlook it. I am referring to the fact that the extent and character of our military capabilities for any future crisis tend to be predetermined by peacetime preparations made long before the event. That is a new situation for us, who have been accustomed to expanding and reshaping our military power when the crisis was already upon us. We have heard much about the "point of no return" in

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our air power build-up, that is, the point at which we commit ourselves to an air combat capability resting exclusively on nuclear weapons. What is less obvious is that this popular phrase merely dramatizes a crisis of decision that pervades our entire military structure. And the decisions we are making now with respect to our military structure will inevitably affect gravely our diplomatic freedom of maneuver in the future.

Decisions have been made and are being made now which will determine whether we can fight limited wars at all, and, if we can, under what circumstances and with what constraints we must fight them. The manner in which the character of any total war of the future is being predetermined by current preparations is even more striking, though less interesting from a political point of view, both because it is less likely and also potentially annihilative.

However, what is abundantly obvious is that the "massive retaliation" threat is becoming rapidly and sharply less significant even as an implicit factor in our foreign policy position. For as the Soviet retaliatory air capability against us continues to grow, it becomes clear that the conditions under which we can hint at a possible use of our strategic air capability against them becomes vastly more circumscribed. There is a strong possibility that it will finally be confined to use only against the threat of direct strategic air attack upon us, which means that it will cease

to be of much significance for a host of lesser contingencies. Yet there is no evidence that there has been any fundamental reorientation of our politico-military outlook on the world since the days when we enjoyed undisputed monopoly of the atomic bomb.

Clearly these are great problems on which political scientists could have much of value to say, if they could get themselves interested enough, which thus far none but a minute handful have done. When we ask ourselves why this is so, a number of reasons come to mind. I shall try to list them and comment on them briefly.

The first and most obvious reason is the security barrier. Training in scholarship and research induces people to seek the boundaries of human knowledge, but not the boundaries between those who know and those who don't. As one whose work makes him privy to "classified" information, I can assert categorically that for the formulation and evaluation of national policy, the information in the public domain is so immeasurably greater in volume and significance than what is kept secret that the latter may well be ignored except for quite special problems. But that is after all only my assertion. The scholar who cannot see both sides of the fence may find my point of view difficult to accept, or even increible. Why so much fuss about secrets if the things kept hidden are marginal rather than fundamental in importance? The answer is that in most instances the secrets are of relevance to technicians and

not to political policy specialists. To be sure, some things of rather grave importance have for a time been kept secret. An outstanding and exceptional example is that of the fallout menace, the existence of which was kept secret for about a year and a half after the "Castle" shot that exposed it. How can the outsider know how exceptional such a case is? Nevertheless, I believe the secrecy barrier much less important than the other factors I shall mention.

A second reason is the fact that the esoteric nature of the military art is commonly exaggerated. Naturally, one does not learn how to be a general by reading books. But it is also true that no general can become really top-flight in his profession without absorbing a kind of knowledge available in books--and there is nothing to keep civilians from reading the same books. In other words, one can learn from books what is consequential about the military art, though very few people attempt to do so. By the word "consequential" I mean, in a very loose sense, the strategic as distinct from the tactical. The military profession, unfortunately, has tended to encourage the civilian to reconcile himself to his own ignorance about military matters, as evidenced by the use of such scorn-laden phrases like "arm-chair strategist." This attitude, which stems largely from an anxiety to retain maximum freedom of decision, is sufficiently ambivalent to break down entirely when it comes to enlisting public support in an inter-service dispute.

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A third reason is very similar to my second, but distinct enough and important enough to warrant separate mention. Technology is rampant everywhere in our society but nowhere more so than in the military art. With respect to problems central to his own profession, the military man himself tends to develop an inferiority complex towards the scientist and the engineer, though he has good defenses against letting his deference get out of hand. Nevertheless, he has to follow the scientists and technicians in some comprehension of a lore which Mahan or Douhet never dreamt of. The civilian who wants to comprehend the military problems of our time has to do likewise. There is, however, among intellectuals in our civilization a deep and sharp division between those who know the equivalent of a good high-school course in physics and those who don't, and I am afraid that political scientists are generally on the side of those who don't. There is, of course, no reason inherent in the subject matter why they need be so lacking in technological understanding, though there may be significant temperamental reasons. Yet when one considers how in little more than a generation the sister field of economics has become populated with young scholars who can discourse easily, and often even purposefully, in mathematical terms, there is some hope that political scientists can be induced to confront a datum in physics without behaving like the horses who saw the first automobiles.

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I should like to mention finally--though not with any thought that I have exhausted the list of relevant factors--the weight of tradition in determining scope and method in political science. At least to one who observes it from the outside, research in the physical sciences seems to be directed by something comparable to free association. The discovery of one fact induces scientists to seek to discover associated facts, and in the process they seem not to be worried over-much about the respectability of the direction in which they are moving. Perhaps those who are inside the process and who are sensitive to such matters are more aware than I of the existence even in the physical sciences of social attitudes favoring or discouraging particular lines of research. No doubt there are fads in research even in physics, though the existence of such fads is probably less important than the question of how rapidly they wax and decay. In political science the favored fixations tend to be too enduring even to be called fads, often lasting for a generation or more.

One of the most enduring attitudes of all has been that which exempts the study of war itself from a field in which scholars are intensely (and quite properly) concerned with the factors that tend to produce or to prevent war. The factors I have already mentioned above are no doubt partly responsible, but there is also some redolence of an attitude that was much more prominent during the inter-war period than now: namely, that the preoccupation with matters military is somehow

immoral in a scholar, or at least not wholly respectable. In the 'twenties and 'thirties one was expected instead to be interested in the finer points of the League of Nations Covenant, which was designed to prevent war. And although that attitude has itself largely disintegrated, its consequences linger on. The military profession is often charged with being unduly conservative, but it cannot begin to compete in that respect with the curriculum designers in our colleges.

I have tried in the foregoing to present neither an indictment nor a justification of political science, but rather an explanation to non-social scientists of some of its peculiarities of scope and method. In doing so I have been at some pains to distinguish between characteristics which I believe are intrinsic, and deficiencies which are remediable. In the latter respect, I have concentrated especially on the failure of political science to cope with the many political problems associated with the nature of modern war, especially in its more novel aspects.

In doing so I may have somewhat distorted the picture by failing to give sufficient credit for admirable work well done. I have indicated that there has been in fact much excellent work in the field; and if I had the time and were willing to test the patience of my listeners, I might even have tried to catalog some of it. But I can plead in extenuation that the H-bomb does raise some oppressively important issues.

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